

# **Report of the October 2003 Meeting of the Astronomy and Physics Working Group**

The Astronomy and Physics working group met on Oct 20 & 21 at NASA Headquarters. Our lead contact is now Jeffrey Hayes. The meeting was attended by Chris Blades, Steve Boggs, Ed Cheng, Mark Devlin, Kathryn Flanagan (co-chair), Dick Miller, Douglas Richstone (chair), Steve Ritz, Eun Suk Seo, Tuck Stebbins, Wilt Sanders, Ted Snow, David Weinberg, Erik Wilkinson and Jonas Zmuidzinas.

We thank NASA for providing a series of informative briefings. We continue to be impressed with the vitality and scope of the R&A Program. We see one very important problem and a few less urgent matters. As usual we have focused on issues that concern us.

## **Stovepiping the R&A Program**

There is one particular concern about changes in R&A that APWG found very distressing. We see very serious potential problems with the pending decision to segregate components of R&A funding into separate SEU and ASO budget lines. Such a division would arbitrarily assign research programs to one theme even though they actually support both. This could prevent excellent programs from being eligible for funding. For example, a theory proposal to study star formation might not be supported because no theory funding would be available under the ASO line; or a proposal to develop UV technology in support of cosmological research might not be funded because it is SEU science but all UV supporting research must be funded under ASO. In short, the planned separation of SEU and ASO funding creates artificial and unnecessary barriers to funding the best and broadest science.

This change also creates new bureaucratic burdens for the discipline scientists who manage R&A programs and reduces their flexibility to reprogram funds within between different R&A activities. This flexibility was sought for years by SScAC as a way to respond to the best ideas in Code S science.

## **The R&A Funding Level**

The APWG is pleased to learn that funding for R&A programs is being increased to account for inflation. We gratefully acknowledge the extraordinary effort that the Division has spent in advocating the R&A program both within and outside of NASA. Nevertheless, we remain concerned about the current level of R&A support. We believe that a robust R&A program is essential to achieving NASA's space science goals.

First, R&A is the "seed corn" that enables future missions to be envisioned and designed. Technology development research produces the innovations that become the basis for these missions, which almost by definition are beyond current capabilities. Sub-orbital

programs, in addition to serving as testbeds for these technological innovations, function as the training ground for the PIs of future missions; many of today's satellite PIs and Project Scientists got their start in these programs. Theoretical research produces the ideas for future missions and guides the design so that they achieve their scientific goals. Advance theoretical work is becoming increasingly important in the design of NASA missions as with WMAP and LISA. Without R&A funding, the space science program will lose vitality in the long term.

Second, R&A is essential to extracting the science from current missions. Laboratory astrophysics experiments are often crucial to even the basic interpretation of data from space missions. As these data become increasingly complex, they demand increasingly sophisticated analyses, which often go far beyond the basic reductions incorporated in mission operation budgets and often require bringing together results from several different missions. Finally, complex data usually do not "tell their own story" --- understanding their scientific implications often requires sophisticated theoretical modeling. Recent examples include inferring the physics of the very early universe from the microwave background fluctuations observed by WMAP and modeling the broad iron K-alpha line in MCG-6-30-15 as emission from a relativistic accretion disk, thus establishing that the gravitational source is a Kerr black hole. Without R&A funding NASA may not come close to realizing the full potential of its investment in current missions. A robust R&A program is essential to achieving NASA's present and future Space Science goals.

Perhaps R&A should be called Mission-Enabling Strategic Analysis which has a good acronym and captures some of the forward-looking nature of the program.

### **R&A Metrics**

We think that a clearer set of metrics for evaluating R&A contributions would also be valuable in making the case for R&A funding. Some metrics would be straightforward to implement, such as number of published papers, numbers of supported undergraduates, graduate students, and postdocs, number of awarded PhDs, and number of E/PO events held. An appropriately designed electronic reporting system could make this kind of record keeping relatively easy. Other metrics, such as developed detector systems or Science News stories, are a bit trickier to keep track of, but also important. At each mission selection (and perhaps again at each mission launch), the R&A-supported technology developments that were important to that mission should be identified and publicized. Similarly, there should be an effort to identify the R&A-supported activities (including technology developments, theory, and data analysis programs) that are connected to each year's Space Science Updates.

### **Helping Scientists Find the Right Program**

We also recognize that the broad nature of the R&A program makes it an easy target for cuts, and that long-term increases in R&A support may need to be tied to specific lines and themes, where they are better protected. The Beyond Einstein Foundations Science

program is a perfect example of this approach, and we applaud its adoption. We hope that this example will be replicated in the future, perhaps with an Origins Foundation Science program. This line- and mission-oriented approach to funding R&A is sound in principle, but it has potential pitfalls: when funds are divided into many small pots, it is difficult for investigators to figure out what activities can be funded and where they should submit proposals, the burden on proposers and reviewers increases, and the grants that can be awarded by any one program may be too small to support even moderately ambitious research programs (involving, say, two years of postdoc support). These problems may be especially severe for analysis efforts that involve multi-wavelength data from different missions, or for topics that involve both SEU and Origins science. We encourage NASA to explore ways to keep the proposal process as transparent as possible, even if the sources of funds multiply, and to allow research programs that are relevant to multiple missions or lines to tap more than one source.

### **The Beyond Einstein Program**

The APWG appreciates that the move of some activities to Beyond Einstein that have been traditionally funded through the R+A will likely have a positive impact on the entire program.

There are several detailed points that should receive special attention. First, any changes should be clearly communicated to all interested potential proposers long before any deadlines (for example, the gravity community should be told that the ADP option for calculations of gravitational radiation waveforms is no longer available). Second, the APWG fears that certain theory proposals will be submitted to the wrong program (BE vs. the re-scoped ATP). The discipline scientists may have to provide some kind of proposal triage to make sure they go to the appropriate panels.

Third, we'd like to understand whether the strategy of using 'theme fundamental science' lines to augment R&A is and will be limited to theory, or whether there will be help for technology, lab astro, and science carriers (balloons and sounding rockets) through this program. In particular, will there be a shift of experimental proposals to the Beyond Einstein program like the shift of theoretical proposals? While the theoretical component of the shift seems straightforward, the experimental portion does not. There are many types of experimental efforts which could support BE. These include detector and instrument development, ground-based work, and sub-orbital work. In fact, it may even be possible to include some lab astro in under this program or a future program. Without clearly defined guidelines there are likely to be problems. There would also be the added complication of reviewing such broad experimental activities.

The additional BEFS funding for these activities for FY04 is about \$6M. In previous years, the theory component of this work has been about \$5.2M per year. There have been recommendations from the Decadal and Senior reviews to double the highly successful theory R+A program. This would leave little money for the experimental efforts. It is clear that the APWG needs more detailed information. Without knowledge of the scope of the BEFS experimental goals, it is difficult to evaluate the appropriateness

of the funding level. However, it seems unlikely that this amount could fund all efforts currently relevant to BE, let alone new work.

### **Full Cost Accounting (FCA):**

Guenter Riegler presented an overview of the implementation of Full Cost Accounting (FCA) at the NASA centers. We understand that full-cost accounting permits, in principle, the proposal and peer-review system to find the most efficient ways to do the best science, and removes (to some extent) hidden costs or inefficiencies. We appreciate that some activities are more efficient at the centers, and others are better undertaken at the universities. Labor costs at universities are often less than at centers, especially if graduate students are involved in the research, while centers often have unique facilities and expertise that cannot be duplicated at universities, and tend to have more continuity in personnel and programs. In the best of worlds, full costing and peer competition would encourage collaborative proposals that combine the strengths of both universities and the centers.

We do (of course) have some worries. Most importantly, given the vulnerability of the R&A program to cuts at OMB, it must be very clearly explained that the apparent increase in the R&A budget that will appear as an inevitable result of FCA is not a real increase. It would be a tragedy if these costs were lopped off. Second, reviewers must receive clear instructions early in the process (in particular, we do not understand whether proposals are to be judged on the basis of science quality or science per dollar). Third, we have some concern about the Centers ability under FCA to compete effectively for outstanding scientists with tenured and tenure-track positions at universities, although we note that operating under FCA JPL has been very successful in the past, and serves as an important existence proof that this whole system can work effectively.

### **Technology Development for Future Missions**

There was one development that we feel quite positive about. Mel Montemerlo's briefing on technology issues, in his new role as APD Technologist, reflected a burst of clear thinking. We feel there has been a considerable decrease in opacity in the strategies APD uses to identify technologies critical for future missions, and a corresponding increase in understanding Code R's motivations and constraints. We look forward to hearing more about efforts to bring along technology developments that align with the out years of the strategic plan and roadmaps.